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the main reason for the development of these towns was to be found in their position of advantage in trading with the more desert regions further south.

Because the poverty ridden peasant or semi-nomad, with no reserve to fall back upon in bad years, cannot now make a living in a semi-arid country is no reason why it could not be done in Roman times when capital was abundant and capitalists controlled the peasant, already half a serf. Only our own days have seen the accumulation of capital comparable to that of the period when the Negeb cities were built, and even now we see many failures—for example, Garden City in Kansas, where just this lack of a reserve has caused the farmer to give up the attempt to carry on his farming in the face of a series of bad years. When Professor Huntington demands that Beersheba and Aujeh "raise good crops every year,"* he demands more than any American farmer, in much more fertile country and with all his argicultural knowledge, has a right to expect.

We have thus tested in detail a considerable proportion of the facts which Professor Huntington has cited to prove the theory of pulsatory climatic changes. Some have proved to be no facts at all, others seem to prove the exact reverse of his theory of the development. In these explanations, he has minimized or entirely omitted recognized causes along geographic, social, economic, and political lines. He seems to have claimed for his theory all facts which at first sight seemed to be in favor of it, without analyzing the antecedents or testing it for modification or rejection. Probability may well be in favor of a gradual movement towards aridity over the entire globe, and there may have been such fluctuations as the theory demands. But it is evident that Professor Huntington has not used his material with adequate care, his periods cannot be shown to agree with such facts as we have, and the historian, for the present at least, cannot use his results in writing his detailed history.

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CLIMATIC CHANGES IN THE NEARER EAST: A REPLY

To the American Geographical Society:

An article such as that of Professor Olmstead in the *Bulletin* is of great value in emphasizing the fact that there are two sides to every question. I recognize that in the earnestness of the pursuit of a new and engrossing theory I am in constant danger of doing what Professor Olmstead accuses me of, namely, claiming for the theory "all facts which at first sight seem to be in favor of it without analyzing the antecedents or testing it for modification or rejection." Therefore I welcome all such criticisms, and am ready to modify my theories so as to bring them into accord with the facts. It must be borne in mind, however, that I have nowhere claimed finality for the climatic curves which I have published, nor for the theories which they represent. The whole conclusion to which the facts set forth in "The Pulse of Asia" and "Palestine and its Transformation" have led me is summed up in the preface to the latter book in the statement that "the climate of the past five thousand years has been subject to numerous changes, (and) these *may have been* a potent factor in the guidance of *some* of the greatest events in history." I emphasize the words "may have been" and "some" because I do not want to be understood as claiming that climatic changes are anything more than one of several important but, as yet, little understood factors, which may perhaps have co-operated with the well-known political, social, and religious factors upon which historians have rightly laid so much emphasis. My attitude toward the whole matter is expressed on page 403 of "Palestine and its Transformation," where I present a curve entitled "Approximate Climatic Fluctuations of the Historic Period." The statement is there made that, "The line representing climate—as here given—makes no claim to finality. The researches of a single year may cause the shifting of a curve a century or more, or may smooth out some minor curve and add another. Yet in its main features I believe that it will stand."

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* Palestine, 282.

The investigations of the two years since these words were written have strongly confirmed me in the opinion expressed above, and Professor Olmstead's article, while most welcome as pointing out certain minor errors, does not in the least alter that conviction. When the smoke of his article clears away, it will appear, I think, that he has merely attacked a minor outpost and not the main fortress, and that, even so, his attack has not been successful. Let us proceed to count the dead and measure the breaches in the walls.

The first point upon which Professor Olmstead lays stress is that I have assumed the existence of trade routes across the Syrian desert in places where there is no evidence of any such thing. He attempts to prove this by informing us that Lugalzaggisi says nothing about the exact routes which he followed, that the sites mentioned by Sargon and Gudea have hitherto been wrongly identified, and that the Amarna letters make no mention of any route going south of that by the Euphrates. But what does this signify? It merely indicates that if scholars disagree about the extent of Lugalzaggisi's conquests and the places mentioned by Sargon and Gudea we must leave those particular pieces of evidence in abeyance. In writing about these matters I followed good authorities, those that are reputed highest, but if later research places their results in doubt, that by no means disproves the theory of climatic changes, nor does it indicate that there were no roads across the desert. It merely leaves the matter unsettled. Curiously enough Professor Olmstead does not see fit to finish this subject in the paragraph under consideration, but defers it until he takes up the discussion of the conditions at about the time of Christ. There, with apparent forgetfulness of what he has said before, he speaks as if it were a well recognized fact that in those days there existed trade routes which now are not passable for caravans. The well and guard-house east of Petra, which I have described on the authority of Carruthers (Palestine, pp. 275-277), seem to be accepted by Professor Olmstead as conclusive proof of an ancient road leading from southern Palestine and Egypt to the Persian Gulf. His comment on it is that "this may very well be the road of twenty days' length which ran from Egypt to Persia." To-day there is no possibility of such a road by reason of lack of water, as the Arabs themselves plainly state and as the explorer Carruthers has well shown. Professor Olmstead further informs us that according to Lucian, who lived from about 120 to 180 A. D., and whose writings belong to the period from 150 to 180 A. D., this road "ran through the desert and was used only by the postal service, an exact parallel to the post-route of our own day from Damascus across the desert to Baghdad." If Professor Olmstead had looked at the diagram of

"Approximate Climatic Fluctuations during the Christian Era" on page 327 of "Palestine and its Transformation," he would have seen that by the time of Lucian there already seems to have been some decrease of rainfall as compared with the time of Christ. In a word, Professor Olmstead's contribution to our knowledge of routes in the Syrian desert comes to this: he throws doubt on some of the identifications of sites and routes which I had supposed to be correct, but which are at best of only slight value because they date back so far that at present any exact knowledge concerning them is impossible. Second, as to the days of Rome's greatness, when, according to the hypothesis under discussion, conditions were drier than in the days of Sargon, but nevertheless much moister than now, he accepts the evidence of the ruined guard-house as proving that a road existed where none can now exist for lack of water; and furthermore he speaks of this as if it were a well-known road. Finally at a period 150 years after Christ, when, according to the hypothesis, aridity had increased somewhat, he says that this road was merely a post road, and adds the unproved statement that it was "an exact parallel to the post route of our own day from Damascus to Baghdad." The Damascus-Baghdad road was used only a few years when the British were trying to establish quick communication with India, but it proved too difficult and was abandoned. It is quite possible that after the days of Lucian the road from Egypt eastward fell into the same state as that of the Damascus road in the nineteenth century; now it is far worse. Professor Olmstead advances nothing which is in any way inconsistent with the theory of pulsatory climatic changes and much which fits that theory and does not fit the theory of climatic uniformity which he is endeavoring to support.

Another of the points to which Professor Olmstead frequently refers is that ancient accounts from the very earliest times indicate that Arabia was always a desert. Of course it was. No one doubts this for a moment, and nowhere have I consciously given any hint to the contrary. Yet Professor Olmstead, when speaking of the supposed roads across the desert, remarks that "to prove that conditions have changed ... we should have been told of villages with indications of agricultural operations where now there are none. But to-day in north Arabia we have such fertile oases as Hail, Tema, Jauf, Khebar, Aneza, Boreda, not one of which can be proved more fertile in antiquity than they now are." This quotation is illuminating in two ways. It shows, first, that the critic is quite as liable to make rash statements as is the criticised. The fertile oases whose names sound so well are miserable little towns scattered over an area about 500 miles square, the only places worth mentioning in 150,000 square miles of desert. I think that I am safe in saying that no one of them has ever been visited by any man who has had the necessary geographical training and also the time and freedom adequately to investigate the matter. I know that I am safe in saying that no account of any such investigation has ever been published. Professor Olmstead's statement that no one of these can be proved to have been more fertile in the past than at present is absolutely without foundation.

The quotation as to oases and as to villages in the desert is illuminating in another way. It shows that in writing upon this subject I have not taken due account of the fact that in the nature of things the historian or the general reader cannot be expected to have much idea of the true nature and causes of climatic phenomena. Professor Olmstead seems to have gained the idea that my theory of climatic changes demands that the desert shall at some time have blossomed as the rose. I have emphasized the fact that the change of climate demanded by the hypothesis is not radical. Here is what I have said on page 261 of "Palestine": "It would be a great advantage to Palestine if the winters were a few degrees colder, so that snow fell more abundantly and stayed longer than now, and if the rainy season were a little longer, so that there would be less danger of drought in the critical seasons of fall planting and spring growth. In the following pages it must be borne in mind that no greater changes of climate than this are postulated . . . If the climate of Palestine during historic times were ever different from what it is to-day, it probably resembled that which would now prevail along the Ægean coast of Asia Minor if the relief of the land and its relation to the sea were like those of Syria." As I was dealing mainly with Palestine I assumed that a statement of the effect of such a change upon the desert was not necessary. Any one possessed of a moderate knowledge of climatology would know that a change of the kind here indicated, while it might have profound effects upon man, would not make the desert habitable. It would merely increase the size of the areas of cultivation along the edges of the desert, it would increase the amount of grass and forage in the desert, it would make the springs more numerous and the wells less likely to dry up, and it would enlarge the available water supply of the oases and thus enable them to support a larger population. In "Palestine and its Transformation" I have given many instances where it seems to me that just these effects have followed. Professor Olmstead has ignored these, which are the strongest buttresses of the theory of pulsatory climatic changes, and has devoted himself to proving that Sinai and Arabia were always deserts, a contention which is perfectly in accord with the theory that he attacks. They were deserts, and the man from the oasis or from Egypt must always have suffered when he tried to cross them, but they were apparently deserts in which the watering places and the forage were somewhat more abundant than now.

I might go on to show other cases in which Professor Olmstead has failed to grasp the full import of what I have written. This may be due to lack of clearness and precision on my part, and if so I must beg for pardon. For instance, in discussing the Exodus Professor Olmstead takes no account of the fact that my climatic curve shows a period of comparative aridity at that time, an aridity which may have had much to do with the movements of the early Hebrews. I have everywhere assumed that while a few Israelites may have come from Egypt, the majority came probably from the desert. Again, Professor Olmstead refers more than once to the vines which I mention at Aujeh in the Negeb. Here is what I have said about the matter: "The [ruined] walls and terraces just described were . . . manifestly intended . . . for olive groves, vineyards, orchards, or vegetable gardens. To-day, except for the few tiny gardens watered by the gasoline pump at Beersheba, no trace of such plants is to be found in the Negeb, unless it be one or two grape vines which the officials of Aujeh have nursed through the droughts." I ought to have added that these vines were only a year old and that most had already died. Even in a desert a man with a bucket and a well can nurse a few vines through a drought.

It would be tedious to go into these matters further, but before turning to something more interesting and conclusive I feel that it is right to call attention to the rash way in which Professor Olmstead has used figures and to his extreme inaccuracy. On page 436 he says: "When Professor Huntington says, 'The trade of Arabia was highly important, although now it is practically nothing,' no one would suspect that this 'practically nothing' meant some $55,000,000 annually." He quotes the Encyclopedia Britannica as his authority. Let us analyze the figures there given. They are as follows:

Aden, £6,000,000, imports and exports combined.
Jidda, £1,405,000 imports, £25,000 exports.
Hoheda, £467,000 imports, £451,000 exports.
Oman, £550,000, of which two-thirds are imports and one-third exports.
Bahrein, £1,900,000 half imports, half exports.

Total, £10,773,000.

Out of a total of nearly £11,000,000 sterling, £6,000,000 belong to Aden. Aden is in truth a part of Arabia, but its business has practically nothing to do with that country. In the first place, it is a British colony, and its sole importance is as a port of call. Its population of over 40,000 has to import everything which it eats or wears. So far as the necessary materials come from Arabia they are included in the figures for other ports as well as for Aden. Moreover, its exports are practically confined to the re-exportation of articles which have been imported. In other words, Aden is merely a place of exchange. Practically its entire trade must be deducted before we can arrive at a true estimate of the real value of the trade of Arabia. Jidda, the next place on our list, is the port of Mecca. The disparity between its exports and imports is enormous. This means that the large amount of importation is due to the necessity of supplying food for the throngs of pilgrims. It is paid for by money which the pilgrims bring from other lands. Therefore except for the £25,000 of actual exports and another £25,000 of imports to balance this, we may fairly say that Jidda has no trade originating in Arabia. Passing on to other towns of our list, we find that all the trade of Hodeda, the port of Yemen, belongs to Arabia unquestionably. Oman, however, is only technically a part of Arabia. In all discussions such as that in which we are now engaged it is so far remote—1,500 miles from the Dead Sea—and looks out so completely toward the eastern ocean and not toward the lands around Palestine that it does not enter into the problem of the relation of the trade of Arabia to the surrounding countries. Bahrein, likewise, is remote and scarcely enters into the problem. It is an island in the Persian Gulf, its exports are largely pearls, and its imports are paid for by the pearls gathered from the sea. Thus it is seen that while Professor Olmstead is technically right in saying that the trade of Arabia amounts to $55,000,000, he is wrong in a much truer sense. His statement is one of those misleading truths which lead to wrong conclusions. So far as the problem now in hand is concerned, the trade of Arabia amounts to less than £1,000,000, and practically all of that is in Yemen, 1,400 miles from Palestine. This small sum of $5,000,000 represents the trade of an area about half as large as the United States. The city of Portland, Maine, whose population amounted to 59,000 in 1910, had a trade of $16,000,000 in 1907 and of $12,200,000 in 1908. It seems justifiable to say that, so far as the problem now before us is concerned, the trade of Arabia is "practically nothing."

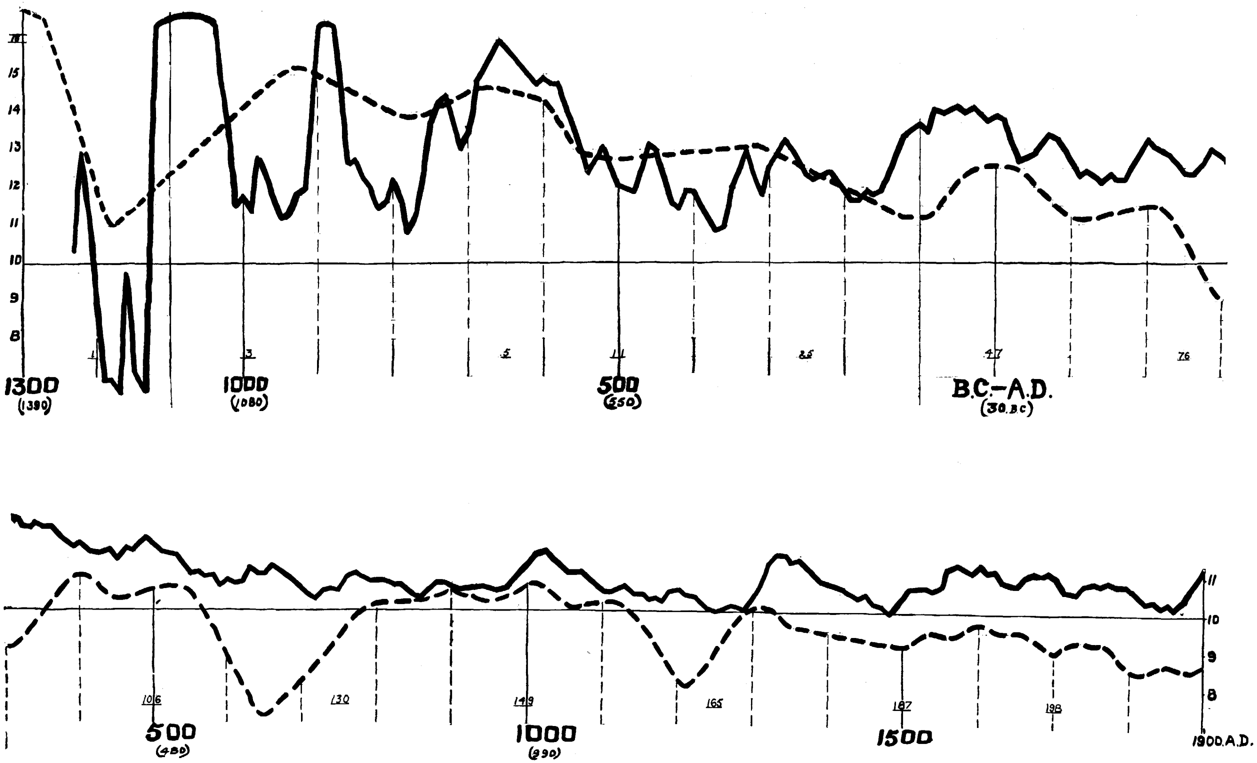
Professor Olmstead is not only careless in his use of figures, but he is far from careful in his representation of the views of others. A comparison of two quotations will illustrate the matter. Professor Olmstead makes the following statement: "[Professor Huntington] accepts the numbers of the Exodus, as well as the general fact of such an event, considers that the data in regard to population given by Deuteronomy is at worst only exaggerated, and even can believe in 'the accuracy of the census of David." Here is what I have actually said:

"It may be admitted that many or even most of the details as to the wanderings of the Israelites are inaccurate, and that there is much exaggeration. It can scarcley be denied, however, that the story has an historical basis . . . In the records handed down to us the number of invaders may have been multiplied ten-fold or twenty-fold, but it must have been large" (p. 271 "Palestine, etc."). "In David's time the population, according to the census which he took, is reported to have been between five and six million. Most authorities agree with Hilderscheid, one of the strongest opponents of the theory of climatic change, who says that although these figures may be regarded as 'in Oriental fashion greatly exaggerated, yet it cannot be doubted that the population of that time was much more numerous and dense than it has now become'" (p. 263). If all Professor Olmstead's statements are on the same plane as those in regard to the present trade of Arabia and my belief in the accuracy of the census of David, his whole contention falls to the ground.

I might go on to discuss other points raised by Professor Olmstead, but the dissection and criticism of another man's arguments are apt to prove wearisome to the reader. Let us turn to something constructive. In the present discussion two points are at issue—first, the verity of my hypothesis as to the actual existence of climatic changes on a large scale and extending over large periods; and second, the possible effect of such changes upon historic events.

This second problem belongs primarily to the historian; the task of the geographer is to establish his own conclusions as a sound basis upon which his historical colleague can work. Therefore I shall turn to the purely physical side of the matter. In order to test the conclusions formed in Asia, I went to California in the summer of 1910 and 1911, under the auspices of the Carnegie Institution of Washington, and among other things made use of a method which was originally suggested by Professor A. E. Douglass of the University of Arizona. In dry regions like Arizona the growth of trees depends almost entirely upon the amount of winter precipitation. If the snow falls heavily during the winter and the rains last well into the spring, the long summer season of drought has compartively little effect, and the trees add to their trunks a thick layer of new wood. In dry seasons the reverse naturally takes place. It is evident then that by measuring the thickness of the rings of annual growth upon the stumps or trunks of trees that have been cut we can determine the relative amount of rainfall at any period during their growth. Accidents will, of course, affect the growth of individual trees, but actual experiments show that when a sufficient number of trees is used the effects of individual idiosyncrasies and even of such things as forest fires disappear, and the curve of growth corresponds closely to that of rainfall. To be sure, certain corrections must be made in order to eliminate the difference in the rate of growth of young trees and old, but the corrections are purely mathematical and can easily be made provided sufficient trees are available.

In California I measured the rings of about 200 of the so-called Big Trees, or *Sequoia gigantea,* high on the slopes of the Sierra Nevadas. The resultant curve is given herewith, the solid line. Horizontal distance represents the course of time, while vertical height represents the rate of growth. Where the curve is high the trees grew fast; where it is low they grew slowly. The numbers in parentheses below the dates at the bottom of the diagram represent the maximum probable error in ascertaining the years. The error may be much less than is indicated; it can scarcely be more. The small numbers within the body



Diagrams showing rate of growth of the "Big Trees" of California at different Periods.

of the diagram represent the number of trees used in preparing the curve. Back of about 500 B. C. the number is so small that the degree of accuracy is not high. From 200 B. C. onward there can be practically no question as to the general accuracy of the results, although further investigation may necessitate minor modifications. There is, however, some question as to whether the low places should not fall somewhat lower than they do, but this is immaterial for our present purpose. It arises from the fact that the final mathematical corrections cannot be fully applied until more trees have been measured. Whatever changes may be made by further investigation and measurement, however, it is practically certain that the general sinuous form of the curve will remain, for a similar sinuosity is characteristic of the curves of growth of numerous other species. Moreover, within the species the sinuous form appears in the same fashion no matter whether one takes one group of trees or another. It seems to indicate, and I may almost say proves, that the climate of California during the past 2,000 years has not only grown drier, but has also been subject to pronounced pulsatory changes.

Beside the curve of the trees from California I have placed another, that of the approximate climatic fluctuations of western and central Asia as given on pages 327 and 403 of "Palestine and its Transformation." This is the visual expression of the theory which Professor Olmstead criticises. The curve is far from final; many portions of it, as I have shown again and again, are based on fragmentary evidence, and will be modified as soon as more data are available. Everything now seems to indicate that each change in the Asiatic curve will bring it more into harmony with that of California. Even as the curves now stand there is a marked degree of similarity, which in some cases, such as the periods centering at the time of Christ and 1000 A. D., extends to details. The lesson of the curves is obvious. We have before us two attempts at a reconstruction of the climatic conditions of the past. One, that of Asia, is based on fragmentary evidence of many kinds and is full of the errors which are inseparable from work of a new kind. It is also full of the errors which arise because of the prepossessions and personal equation of the investigator. The other is a continuous record based on purely mathematical considerations. It cannot be changed by the investigator's private opinion and predilections. Yet the two curves in the main agree. They both indicate that the climate of the earth is subject to changes, and that those changes are of a pulsatory nature. Moreover, the changes as indicated in the California curve are of sufficient intensity to cause serious modifications in the economic conditions of the inhabitants of the country. Therefore they must have had distinct and recognizable historic results. Farther than this I do not now care to go. I may have misinterpreted the results in many cases, but little by little we are approaching the point of certainty as to the nature and date of climatic pulsations. With each step we are drawing nearer to the point where we, the geographers, can say to the historians, "Here are the definite facts as to changes of climate. What results have they produced?"

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